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tures of the organism rather than on environmental factors. And however complex this inner tendency to action is, and however much of its complexity and significance it may owe, at any moment of its history, to the modifying influences of education or training, it presupposes a minimum core or foundation in the inherited structures of the organism without which it could not have had a beginning. The minimum core or foundation thus presupposed, in so far as it is inborn, and in so far as it makes possible significant interactions with the environment, is, it seems to me, on the basis of Mr. Kuo's own argument, deserving of the name "instinct."

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THE MODIFICATION OF INSTINCT

IN an article on "The Modification of Instinct from the Standpoint of Social Psychology," published in the *Psychological Review*, volume 27, 1920, pp. 247-69, I took the position that instinct could be modified by habits formed prior to the instinct's appearance. As a partial support for this view, I cited the following observation made by C. O. Whitman upon pigeons:

If a bird of one species is hatched and reared by a wholly different species, it is very apt when fully grown to prefer to mate with the species under which it has been reared. For example, a male passenger-pigeon that was reared with ring-doves and had remained with that species was ever ready, when fully grown, to mate with any ring-dove, but could never be induced to mate with one of his own species. I kept him away from ring-doves a whole season, in order to see what could be accomplished in the way of getting him mated finally with his own species, but he would never make any advances to the females, and whenever a ring-dove was seen or heard in the yard he was at once attentive.

Professor H. A. Carr, in editing the Whitman manuscript, also directed attention to the principle involved. Since the publication of my paper, this interpretation has been questioned first by Professor James Leuba in private correspondence and last by Mr. Zing Yang Kuo, in a most interesting paper upon "Giving up Instincts in Psychology," this JOURNAL, Volume 18, 1921, pp. 656-7. The criticism urged is that the behavior of the pigeons so modified was not an instinct but a habit. The passenger-pigeons' choices of mates from their own species are themselves, so it is said, the result of training and association and are not innate. Therefore, so the conclusion runs, we do not have the modification of an instinct, but merely the supplanting of one habit by another.

The proper understanding of much human and animal behavior depends to such an extent upon the principle here involved that I wish to suggest the answer to the problem as I see it:

For the principle concerned, it makes no difference whether the specific choice of mates by normally reared passenger-pigeons is chiefly inherited or acquired. The modification brought about is of the same general type as that found in the conditioned reflex, save for the temporal location of the modifying influences. Whitman's observation gives us a case which is scientifically described and convenient to use, but illustrations might be drawn as well from man's sex choices, from the field of sex education, and elsewhere. The sex instinct consists of those motor and glandular activities whose occurrence is due to an inherited synaptic connection giving certain sensory impulses ready access to the necessary final common paths. The instinct is certain behavior set up in muscles and glands. It is not a stimulus, nor yet an inherited synaptic connection, but is aroused by the former and controlled by the latter. Considered most accurately the *instinct* is not modified unless effector activities are changed. We are considering, however, as others have considered, the modification of the total inherited stimulus-response situation. The immediate problem is whether or not habitual associations formed prior to the appearance of the instinct have set certain stimuli as the ones which will be effective in arousing the instinct.

The detailed stimuli arousing sex behavior are unknown. In general, however, two classes of stimuli are involved: visceral sensory impulses corresponding to appetite, or desire, on the conscious side; and somatic sensory impulses aroused by the external object or by the symbol which represents it. It is probable that in many animals there is not a high degree of specificity on the somatic sensory side and that these factors are supplied to a high degree by the experience of the individual. However, the chance selection of somatic avenues seems to be weighted in favor of cutaneous, olfactory and possibly visual stimulation, *i.e.*, the somatic avenues do not seem to be equally open. On the visceral sensory side there seems to be an undoubted native connection between internal secretions and the appearance of sex responses in the somatic and visceral effectors (just as there is a connection between stomach contractions and feeding activities in normal animals). Exercise seems able only to vary this visceral sensory factor and so affect the intensity of the appetite. Whether or not this is actually the case, I do not know. (A diagram indicating the coexistence of the two types of sensory avenues is presented on page 253 of my paper above cited.)

What Whitman's observations show is that prior to the appearance of that typical form of response known as sex behavior the associations established within the lifetime of the pigeon have changed the stimuli which will later help in eliciting the response by

varying the synaptic connections on the somatic sensory side of the arc. The contrast between mating with members of the same species in the one case and with members of another species in the other case gives the essential fact. In neither instance need the stimulus be connected with the response through synapses set by heredity in order that the modification of instinctive behavior conform to the principle stated. If experiment should show that there is no somatic afferent connection set by heredity to arouse the mating response, the stimulus-response fact would of course be different from what I am inclined to assume, *viz.*, that along with the inherited motor grouping goes a more or less definite nervous organization favoring certain somatic stimuli. But such results, if secured, would only further confirm the fact that associations formed prior to the appearance of the instinct may modify it (either on the motor or on the sensory side) when it does appear. In the food-getting instinct in chicks one has an instinct similar to the sex instinct in that the sensory components of each include both somatic and visceral factors and in that the somatic stimuli are less definite than they later become. The chick pecks at first in response to visual stimulation from any small object or in response to an overpowering appetite in the absence of the proper external stimulus. With practise the somatic stimuli become confined largely to food objects. The significant difference between the food-getting and the mate-getting responses is that one appears shortly after birth and the other much later. The food-getting instinct therefore offers little opportunity for modification by experience either on the sensory or on the motor side prior to its appearance.

I do not regard it as the function of this paper to discuss the question of whether or not instincts do exist. The psychologists who are questioning the existence of inherited forms of response may do the science a service in forcing a more definite use of terms, but so far the prospects of attaining their avowed goal do not seem encouraging. To disprove the existence of instincts, one must either disprove the existence of reflexes or prove that there is a significant difference in kind between the behavior termed instinct and that termed reflex. Mr. Kuo discounts the idea of the existence of instincts partly because the behavior in question involves the coördination of simpler responses, and coördination he holds to be the result of habit. However, physiological work indicates that even the simplest reflexes are coördinated activities. The author also disputes the existence of delayed instincts, inherited forms of behavior appearing at varying intervals after birth. Various angles of this question have been long discussed, but I cite in opposition to Mr. Kuo's view only Lloyd Morgan's somewhat theoretical dis-

cussion of the moorhen's first dive and Yerkes's and Bloomfield's experimental observations on the behavior of kittens in killing mice. In order to disprove the existence of delayed instincts, it is not sufficient to indicate that elements of the response have been exercised before. It is necessary to account for the somewhat sudden grouping of the elements into a significantly new response under conditions where the influence of habit formation is experimentally controlled and negligible.

The opponents of instinct will also have a very considerable difficulty in handling such data, meager though it is, as that presented by Yerkes on the inheritance of savageness and wildness in rats and by Whitman on the hybridization of pigeon behavior.

There are other interesting—but I think seriously mistaken—points both in Mr. Kuo's paper and in other recent papers couched in a similar vein. My purpose, however, is merely to remove the misapprehension which has come to my notice with reference to an earlier proposition, *viz.*, that associations formed prior to the appearance of an instinct may modify the instinct when it appears.

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BOOK REVIEWS

Mysticism, Freudianism and Scientific Psychology. KNIGHT DUNLAP. St. Louis: C. V. Mosby Company, 1920. Pp. 173.

There are two main points of interest in this stimulating essay: the pigeonholing of the Freudians with the mystics; and the reflex arc concept applied to perception and to the association of ideas.

Mysticism is illustrated by quotations from Plotinus and Dionysius, as well as from Maeterlinck and other modern mystics. It pretends to reach a "third kind of knowledge," additional to that gained through the senses and through inference.

This third and highest type of knowledge is to be had only in rare moments of absorption in an adored object, and is ineffable and incommunicable. The author concludes that this "knowledge" amounts to emotion, pure and simple, with the sex element strong in the emotional complex. But such emotional experience has no claims to scientific recognition as a source of knowledge.

"Pseudo-mysticism," exemplified by belief in spirit communication and telepathy, differs from the genuine article in pretending to employ logical inference in reaching its conclusions, but its logic is constantly vitiated by the use of ambiguous middle terms,